

Morgan wrote me asking if I could buy my own microscope.  
Sharp thinks Morgan should have some more obtained for us there.  
What is the financial situation? Can they afford to buy any?  
Sharp is in for my taking one from Cornell.  
Could you let me know - ask Sterling

July 30, 1931

Dear Charlie,

[Take a week to read this letter]

Your letter just came. I was delighted with all the news. It shows great progress. I had been on the verge of writing you to tell you what I have been doing & I felt you would like to know.

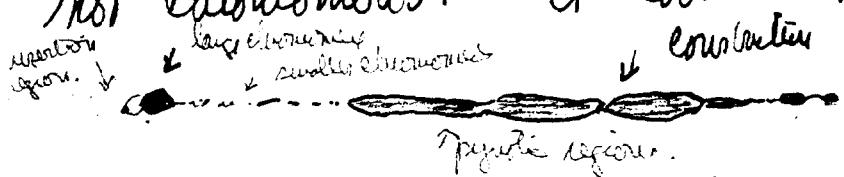
I was especially pleased about the  $K_3 + \text{ta}$ . Just how soon will the interchange occur? I think #7 will be an easy one for nephrons as I believe it has a stop check marker near the insertion region. I have it checked this but will this winter. You can find which part gives the peculiar #7 terminal appearance.

Also - *Japonicus* - a great find. One of the earliest known genera no place for it. It would be nice to have it one #8. Will you make the cross? I have not crossed  $R_1$ ,  $G$  m + long interchange class of  $R_2$ .

Am interested in the translocations involving the satellite chromosome. Have you found the position?

or *ambonyx sens*

Have sent the paper in to the proceedings. Thanks for your P.S. in your letter. I still feel guilty about it.

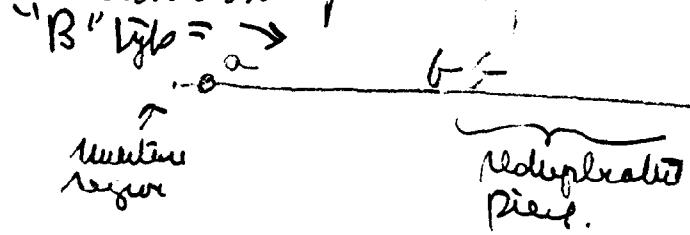
Concerning my work this summer - Several interesting points. I don't know whether to begin chronologically or jump into the mudobs - First - I think I have some of the "B" type chromosomes of Randolph's solved. I ran into it by chance in some of Stacker's material & did not recognize all this was until I had some of the story. The chromosome is very peculiar, unlike any other morphous chromosome. It is almost entirely pyrastic - hardly has the kent region but not chromosomes. It looks like this in prophase - When  2 are present they usually

Synapse together along their lengths to form a double structure as above but may squash with these, double around on itself. That is the reason Randolph found some figures with "10" + 2 numbers instead of 11". When 3 "B" types are present the synapsis is fair great!

②

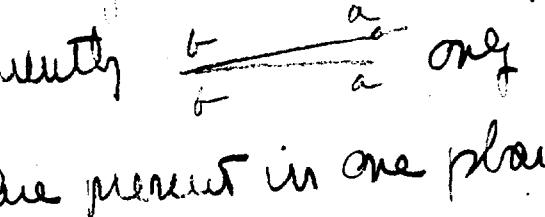
on basis of  
my interpretation

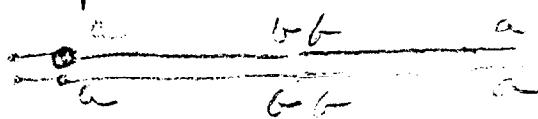
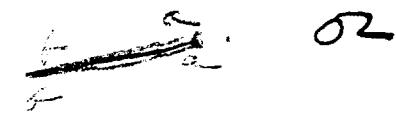
It forms most peculiar figures but none unexpected. The most frequent is a T-form which puzzles me considerably when I start to figure it out. Now this is a 23 chromosome plant first.

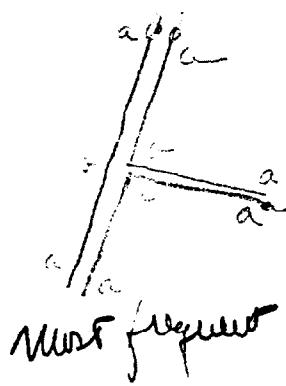
"B" type = 

(2) Interpretation

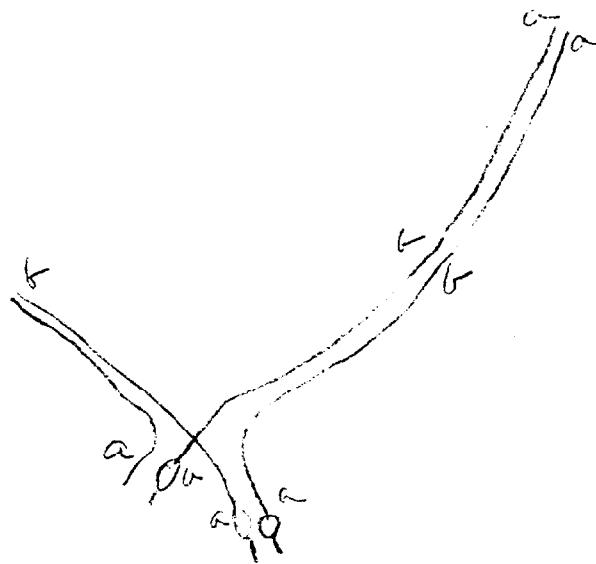
$a \rightarrow$  very occasionally in prophase  
 $=$  (rarely)

a straight bivalenter is formed (21 ch. plant) but more frequently  one close synapsis, not a V. When 3 are present in one plant get three varieties in prophase

 + 



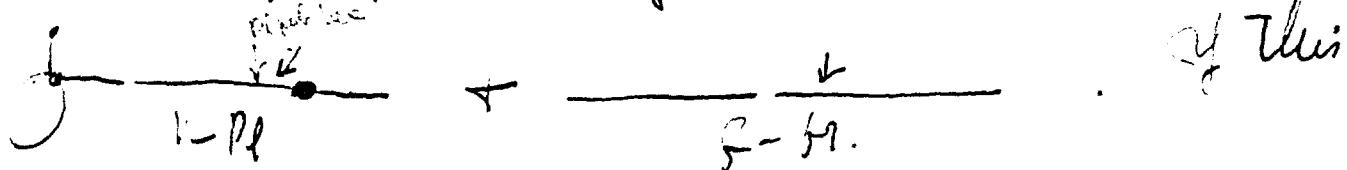
O2



↗ These figures are clear

Synapsis is only 2-by-2  
Forms nine crossovers at different levels.

The 2nd. I looked at some  $\square_5$  morphs to see about the eln. involved in  $\square_5$  to check on  $t-f$  +  $f-6r$ .  
 size ~~in part~~. Wanted to get morphological slope for trivives.  
 Report of Cooper's + Brink's was not complete enough. I  
 found the peak occurred at long arm of satellit  
 chromosome (about region of  $pl$  gene, see below) + first  
 below insertion region on long arm of. I believe, the  
 $f-6r$  chromosome. What is the slope on this eln? If  
 looks so me as if the longest chromosome were involved - i.e.,  
 $f-6r$  + not  $t-f$ . The one figure I had was beautiful. All  
 the other eln. in the cell were very good except 2 which I  
 could make out partly. These looked like the longest.  
 Morphologically (i.e., size + arms) this fit in better with  
 $f-6r$ . Much better than  $t-f$ . Breaks occurred about

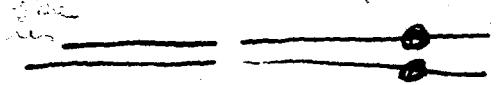


turn out to be  $f-6r$ , morphology of this eln is ok.

This was the right size & morphology for the  $\delta$  Lg chs.  
It had a peculiar marker ~~that~~ near the insertion  
region which allowed me to spot the chromosome in  
this culture. Prophase figures looked like this:



A second Lg plant in water culture had same chs. markers  
only a shorter segment. Normal Ls plants had no  
loss in this chromosome.



B is probably on the long arm near the cent. I am  
first working it out now.

Now for pl - In a similar way Pl  $\delta$  x pl $\gamma$ ,  
 $\times$  rayed embryos, picked out pl plants. Found one  
which showed a deficiency of about  $\frac{1}{2}$  the long arm.  
Therefore pl was in the lower part of long arm of satellite  
chromosomes. Found a 2nd plant that was pl. It  
showed an intercalary between satellite chs. & another  
ch. which occurs at about the same position as is  
D<sub>5</sub> - i.e., just above the knot <sup>on long arm</sup>. If pl was  
knocked out or injured in the intercalary there should be

Rid air from my list of Topics & had made out but this  
letter got too lengthy as it is. I won't blame you for not trying it over

No crossing over between pl & interchanges for pl is probably  
at interchange point. One other pl plant was examined.  
It showed no visible alteration. We shall test it for a  
non-visible deficiency or mutation.

Spent for the major part of the summer work. I  
am looking over a number of defective plants & find many  
types of alterations. Went to attack the so-called recessives  
& standards. In plant characters they are very infrequent.

Dr. Parker & Harriet drove out here in my car.  
I had left it in Ichaca. They thought I needed it &  
drove it out to me. I was delighted to see them for the  
associates here, except for Stebbins, have not been exciting at all.  
It is a one-man show on his part.

The summer here has been hot & no rain. The  
plants suffer. The methods of irrigation are different  
& costly.

Heard the news of Marion & Beadle there.  
Harriet via Rhoads from Emerson who had letters  
from Beadle.

If anything turns up of particular interest  
shall let you know - regards & all -  
Ruf.